training? There is danger of setting to work a mental machine without giving it suitable material upon which it may operate, and it reacts upon itself, resulting in a sort of mental chaos. An active mind, turned in upon itself, without any valuable objective material, certainly can never reach any very reliable results. It is the trained scientific spirit which recognizes that it is dangerous to stray away very far from the facts, and that the farther one strays away the more dangerous it becomes, and almost inevitably leads to self-deception.

It is such an attitude of mind that scientific training is contributing to the service of mankind. This does not mean that all scientific men exhibit this attitude to the full, but that it is their ideal. This ideal has realized some tremendous results during the last half century, and there is every evidence that it is accumulating momentum for a much larger expression. Compared with this contribution, the material usefulness of science seems tawdry. In general, the world's standards of usefulness are tawdry, but education ought to correct them rather than maintain them.

The conclusion is that all science is immeasurably useful, from fundamental to superficial, on the material plane and on the intellectual plane; and that in these two regions of human need it is the most valuable practical asset the world possesses.

JOHN M. COULTER

BOTANY IN ITS RELATIONS TO AGRICULTURAL ADVANCEMENT[1]

FEW things are more interesting to one of a philosophic cast of mind, especially if he be something of a botanist or agriculturist, than a growing collection of plant varieties. However sluggish of intellect one may be, such a collection—representing forms developed in the long history of the cultivator's art—is sure to excite one's interest regarding their origin. At first thought it would seem that as practically all of the numerous varieties that exist in cultivated plants have been developed as it were under the eye of the grower, we should have a pretty clear understanding and agreement as to their mode of origin. Yet few subjects have proved more perplexing. The stock answer of the breeder or gardener to one's inquiries is usually embodied in the words *sports* and *hybrids*. Is this answer adequate? The enormous importance of the subject, it would seem, should have incited the most intensive study into the problem. Few plants in their ordinary wild forms will repay cultivation. It is only through their improvement that a permanent agriculture became possible. The very baffling nature of the problems presented, instead of attracting students, seems to have repelled them. Systematic botanists have looked upon cultivated plant varieties as artificial products—useful, no doubt, but utterly subversive to notions of classification obtained from plants in their natural habitats. Therefore, they have been neglected and no plants are so rare in museum collections as our common cultivated ones. Such a thing as a reasonably complete herbarium of cultivated plant varieties nowhere exists. The natural result of this has been that the systematic botany of cultivated plants is in woeful confusion. As a rule, numerous botanical species have been based on purely agricultural varieties, but in some cases the opposite extreme is found and perfectly distinct species are confused as garden varieties. As a natural consequence of this neglect by botanists, the great mass of information we have concerning any cultivated plant is largely

[1] Address of the retiring president before the Botanical Society of Washington, March 5, 1910.

the work of men of little or no botanical training.

With the establishment of the numerous agricultural experiment stations in all parts of the world, the doors were opened wide to scientific men to work for the advancement of agriculture. It is instructive to review the general trend of what took place in the fields of agronomy and horticulture, which, broadly speaking, not only cover the whole subject of crop plants, but soils as well. Generally speaking, there are four potent and more or less controllable factors which affect the yield of crops. These are *tillage, fertilizers, rotations* and *variety of plant.* To these might be added the prevention of loss by diseases or insects. Broadly speaking, three types of scientific men went into agronomic work. First, those who were interested in the study of fertility. For the most part, these men were and are chemists and they have studied their problem largely or wholly from a chemical standpoint. Probably as a result of their chemical training the field plot work of these investigators is by far the most accurate agronomic field work conducted. The theoretical side of the subject of soil fertility has recently been stimulated by vigorous attacks on the long-accepted theory of available plant food—an explanation so luminously simple that a few pages of text sufficed to tell the whole story. It may devoutly be hoped that a renewed activity in the study of fertility may stimulate botanical work on the nutrition side of the problem—which is pretty nearly where Sachs left it forty years ago. The second class of scientific men who were attracted to agronomic work were botanists. In large measure, these men undertook investigations dealing with plant diseases, with the end in view of preventing or curtailing the serious losses resulting from such causes. The results of their work furnish the best contributions that botany has thus far conferred on agriculture in this country. So far as field crops are concerned, there are decided limitations to the use of any direct preventive methods such as spraying. As a natural result, investigators of the diseases of such plants were forced to adopt one of two lines of approach to the solution of the problems involved. They could either seek for immune or resistant varieties or they could make a comprehensive study of the crop and the disease and endeavor by such indirect methods as rotations to curtail the disease loss. In either case the result was that the pristine pathologist often graduated into an agronomist. The third class of men who went into crop investigations were generally termed agriculturists and horticulturists. They constituted by far the most diverse group. In a few cases they were simply good farmers. In some cases they were men of very broad training. For the most part they were men with good general equipment. To these men fell the great bulk of the field work involving principally investigations into tillage, rotations and the testing of crop varieties. It thus fell largely to this third class to investigate the complex problems of plant varieties. Even in the few cases where experiment-station agriculturists and horticulturists had good botanical training, the diverse problems facing them as well as paucity of literature gave little opportunity for far-reaching studies. Generally speaking, one of two plans was pursued. In the one case a series of varieties was grown, and all but a few of the apparently most promising were discarded without further ado. In the other case more or less full information was

published regarding each of the varieties tested. Further investigations have clearly revealed the very superficial nature of most of these varietal studies. In general, the collections consisted of such varieties as could be gathered locally and through seedsmen. In only a few cases have specimens been preserved, so that it is not possible now to verify or determine the varieties grown, though in many cases it is certain from the notes that the variety published on was not true to name. There has thus been placed on record a mass of misinformation regarding many varieties. In my opinion, at least fifty per cent. of the varieties that have been published upon are either untrue to name or unidentifiable. I hope I may not seem to be pessimistic in portraying the present status of much of the published information on crop varieties. It is the natural result of neglect by men of proper training to do accurate work of a purely botanical character. As an indirect result of this failure by botanists to apply their trained skill to the problems of agriculture, especially as concerns knowledge of crop varieties, there has arisen the idea that training in systematic botany can not be of particular assistance to agriculture. Therefore, it has all but disappeared from college curricula at least in a form to train students to know plants. Few agronomists and horticulturists graduating to-day from our agricultural colleges are well trained in botany—indeed so far as I know no college is training botanists to enter agricultural work, excepting along pathological lines.

I do not feel that I should be justified in thus painting so gloomy a picture of botany in its relation to agriculture, if the recent trend of things did not indicate that better times were coming—indeed are here. There was one field of work that both botanists and agriculturists entered upon in the course of their investigations that has brought them together, namely, plant breeding. It is a happy coincidence that at practically the same time the interest of all biologists has been stimulated to renewed interest in the problems of variation and heredity. The practical results already obtained by plant breeders is an earnest of what may reasonably be further expected. Incidentally but inevitably, the work of the plant breeder has stimulated interest in the matter of existing crop varieties as well as in the principles underlying variation and heredity. Breeding is, after all, largely the production of new varieties. Thus far, breeders have used for the most part locally established varieties as the basis of the work. This is sound as far as it goes, as the local varieties undoubtedly represent the best adapted of those tried, the poorer sorts having been discarded. It is safe to say, however, that but a small per cent. of existent varieties have been tried in most places—so that there may easily exist varieties superior at least in certain characteristics. A realization of this has led to a clearer appreciation of the value of a comprehensive study of the whole botany of our principal crop plants. This does not mean merely a categorical list of existent varieties—which it is evident can be indefinitely increased by hybridizing—but a sufficiently exhaustive study so that we may thoroughly understand the characteristics, both good and poor, that are available to the breeder. The task is by no means an easy one. In the first place, the number of varieties in all our crop plants is far greater than has commonly been realized. For example, there are probably about 2,000 varieties of wheat, 1,000 of beans, 5,000 of apples, 200 of sor-

ghums, etc. What is needed is not so much descriptions and detailed classification of these varieties, as a classification and understanding of their principal hereditary characteristics. In other words, the knowledge of them needs to be arranged not only with regard to the existing forms, but also as far as possible with regard to their characters and potentialities. Such a monograph does not exist for a single one of our principal crops, though there is an increasing number of contributions to the subject. The field is a vast one in which there is not only a great work to be done in compiling what is known of our cultivated plants, but a greater one in clearing up the many problems concerning their origin.

In a very different way plant breeding is beginning to do much to better agronomic methods. I have before stated that the most accurate plot work being done in this country is by the plots devoted to fertility investigations. How accurate are these? Hall, of Rothamstead, thinks no results with fertilizers are at all trustworthy unless the yield difference is at least 10 per cent. In much of the American breeding work going on 10 per cent. increase by selection would be deemed good progress. The question is, can any feasible system of trial plots measure accurately such a difference? Very recently several men have looked into this subject, more or less independently. The most comprehensive work has been done by Lehmann at the Mysore Experiment Station, India. Similar work has been done by Lyon at Cornell, Montgomery at Nebraska, Shoesmith in Ohio and Smith at Illinois. All of these investigators find a surprising difference in plots due to differences in soil. On what was considered the most uniform soil at the Nebraska Experiment Station the variation between plots on one acre was 35 per cent.—a much greater difference than the breeder of wheat expects to get. Lehmann found differences varying from 0 to 300 per cent. —and further that on many plots the difference was increased or diminished according to the season. He proposes to use in his work with fertilizers only the plots that give uniform results for at least two similar seasons, a method that he calls *standardization*. In this country agronomists have used mainly the system of check plots—a system which it now appears may be absolutely misleading. Indeed, a study of the check plot records in various experiments shows that they vary in just the way that Lehmann found his plots to vary.

Some American agronomists are employing the method of duplicate plots—a plan that is rapidly growing in favor. The number of duplications for the most accurate work will necessarily vary according to the evenness of the soil, four to six duplications apparently being necessary for very accurate results even on fairly uniform soil. The subject is, however, one that needs much additional investigation, as the disturbing effects of soil inequalities have evidently been greatly underestimated.

The results of plant breeding seem likely, therefore, to have a profound effect on agronomy as a whole, demanding as it does both the most accurate plot methods to determine relative yields and a much more intensive knowledge of our crop plants—the material with which breeding must work.

There is still another botanical method that needs to be brought more intensively into agronomy—namely, the method of pure cultures, which has brought so great results in our knowledge of the lower plants. It is this method that enabled

Mendel to discover the phenomena that bear his name. Practical plant breeders now generally use the plant-to-row or centgener method in comparing the value of selected plants. It is probably due to the non-use of such careful methods that the origin of most cultivated varieties is so obscure. In many cases, a so-called sport or hybrid turns out to be a well-known thing—in all probability the result of a stray seed. This is perhaps unavoidable, as the business of the seed grower does not readily lend itself to accurate scientific methods.

Of late years our knowledge concerning hybrids and the behavior of characters in hybrids has increased greatly due to the rediscovery of Mendel's laws and the immense amount of splendid investigation which was thus stimulated. No more admirable body of work has ever been done than that of the Mendelists. If it continues as rapidly as it has we may soon expect to know approximately the extent to which hybridizing is a factor in the evolution of our cultivated plants. While the methods of the practical breeder are perhaps necessarily different or at least less accurate than those of the scientific breeder, yet the results of the scientific work are already having profound effect on practical methods.

Without at all minimizing the fruitful results and greater promises of Mendelian investigations, the subject of sports is to both the breeder and the evolutionist a matter of far greater moment. Certainly our knowledge concerning sports is far less than that of hybrids. The more enthusiastic Mendelists have evinced some disposition to deny the existence of "sports" in the commonly accepted sense and would explain them as the result of some previous, even remote, cross. But it is self-evident that hybrids presuppose the existence of two different things to cross, and sporting is supposed to be one method by which a distinct form more or less suddenly arises. Let us examine carefully the evidence regarding "sports." Bud sports, where one branch of a plant is different from the rest, occurring commonly as variations with differently colored flowers, different leaves, etc., are well known. There can be no question as to the origin of the sport here, though to be sure the parent plant may be a cross or hybrid. Seed sports are supposed to arise in an analogous manner. The general occurrence of certain types of assumed sports is strong argument in favor of their actuality. Thus, white-flowered variants are known in practically all plants with normally red or blue flowers; cut-leaved varieties are very common and generally distributed among the plant families; dwarf varieties occur in numerous species, as do smooth varieties in hairy species and vice versa. The logical inference is that the difference is due in each case to the same underlying cause. In some cases the origin of these sports is a matter of definite record, as in the case of the cut-leaved form of *Chelidonium majus*, the globose-podded form of shepherd's purse and others. In the white-flowered form of bleeding heart—its only variant—previous hybridization seems clearly excluded by the absence of any related form that will cross with it. Many such cases can be enumerated and tend to uphold clearly the gardener's idea of sports. But what are these sports, and how do they arise? Apart from the fundamental idea that they are large and permanent variations, breeders and gardeners in general attach three other ideas, namely, that high nutrition and other extreme conditions favor sporting; that many plants must be

cultivated a long time before sporting is induced, and that in any case sports are actually or relatively very rare. Will these ideas stand the test of scientific scrutiny experiments? It is evident that these problems are of high importance both to evolutionists and to agriculturists. De Vries with his *Œnotheras* and his theory of mutation as the chief factor in evolution has particularly interested the scientific world in these phenomena. He has worked out in great detail the facts of variation as they occur in the evening primrose and makes a strong case for his theory. Recent cytological study of the *Œnothera* mutants or variants shows that one of them has twice as many chromosomes as the others; in other words, that this mutant at least has suffered a pronounced change in its hereditary mechanism. It is only natural that this should at once have aroused the suggestion that perhaps all sports or mutants are the result of more or less marked derangement of the hereditary mechanism, by which a character or factor of some sort is gained or lost. MacDougal's work in subjecting very young ovules to chemical influences, and Gager's similar experiments with radium emanations, are also reported to have yielded marked variations, perhaps sports. Tower also secured true sports in increased numbers from his Colorado potato beetles by subjecting them to untoward conditions of heat and moisture during breeding. In this case, however, all the sports secured were previously found occurring naturally. There is a tempting subject here for speculation—indeed one that has been assiduously tilled, but to follow it up will lead us too far afield. The limited historical and experimental evidence of a critical character clearly upholds, however, the reality of sports.

It is an illuminating fact that most of the information concerning the origin of cultivated plants and animals is that brought together long ago by Darwin. Recently De Vries has gathered much additional data. Both these men sought the facts primarily in support of a theory. Scientific men are usually more concerned in finding an explanation of phenomena than in gathering the facts. But we can not all be philosophers and theorists—indeed, the principal difficulty with biological science is that we have a plethora of theory and a dearth of critical facts. Especially is this true in the subject of biological evolution, where nearly every possible guess and combination of guesses as to the actual method of evolution has been made. Where such guesses or theories stimulate additional inquiry they are valuable—otherwise, they are useful only to practise mental gymnastics. It is the great merit of many recent investigators, De Vries in particular, that they emphasize the importance of experimentation. De Vries's work bristles with suggestive lines of experimentation mostly bearing on the subject of the origin of cultivated plants, and nearly all of practical importance in agriculture as well of great interest in themselves. If any one believes that there is any immediate likelihood of biologists agreeing on evolution, all he has to do, using the slang of the day, is to start something. However much agreement there may be on the facts—there is sure to be violent disagreement on the interpretation of the facts. For example, De Vries and others believe that sports which usually breed true from the start are intrinsically different from ordinary or fluctuating variations induced by soil or otherwise and which have no effect on the offspring. On the other hand, Tower, who has conducted extensive investiga-

tions in the evolution of the Colorado potato beetle and its relations—work comparable to that of De Vries on *Œnothera*—argues strongly to show that his sports or mutations differ from fluctuations only in degree, not in kind. By definition, if the variant transmits its characters fully it is a mutation or sport; if not at all, it is a fluctuation. But many supposedly fluctuating variants transmit their characters in large part at least temporarily. Thus peas grown on warm or sandy soils are said to become mature earlier than the same variety planted on colder soils—and this difference is transmitted at least to their immediate progeny. It is believed to be in virtue of this supposed type of variation that northern grown seeds like corn often possess increased earliness when planted south; that continued selection as in sugar beets is necessitated to keep the plants to a high standard. Such plants clearly transmit to their progeny characters limited in both amount and duration. Are they then fluctuations or mutations? Those who hold that fluctuations have no effect at all on heredity, suggest that the sugar beet and kindred cases may represent complex polyhybrids continually breaking up and that rigid selection would, therefore, result in securing pure constant lines with high sugar content. Many mutations are at first partial, as in the cases of many double flowers. The first suggestion of doubling is often only a single additional petal. In the progeny of this individual some with more petals nearly always occur—and the process eventually results in full doubling. The general progress in these cases is seemingly parallel to what occurs in securing the pure lines out of a complex hybrid. A similar case if true is found in Burbank's red *Eschscholtzia*—the first hint of which was a red streak in the petals of a yellow sort. By continued selection the pure red was isolated. Professor Setchell tells me, however, that red-flowered eschscholtzias occur wild in certain localities in California. There is room for much discussion on all these points—but their settlement requires a larger body of critical facts than are yet available. There are plenty of gardeners' accounts of such phenomena to be had and they are probably true, but they do not possess scientific accuracy. Along these lines there is presented an alluring field of botanical work.

A clearer understanding of the different types or degrees of variation is most important. De Vries would recognize only three types, namely, fluctuations, mutations and ever-sporting plants. The latter include mostly plants with variegated leaves or flowers—which also constantly bear part of their leaves or flowers without variegation. A common example is the variegated-flowered larkspur. The azaleas with flowers on some branches red, on others white or striped, offer perhaps a similar phenomenon.

It is quite certain that such a classification simplifies the matter too much. Johannsen's work with beans clearly shows that mutations are often very small, even minute—but they are inherited—while similar variations not inherited are considered fluctuations.

De Vries's compilation of available evidence on the origin of plant sports tends to uphold in general the idea of the gardeners—namely, that sports are comparatively rare; that unusual conditions, especially of nutrition, favor their occurrence; and that often a plant must be cultivated a long time before it will sport. His evidence further shows that in some cases breeders sought out natural sports—and merely intensified their characteristics by cultivation. Whether De Vries's

theories are correct or not, wholly or partly, is of far less importance to agriculture than the stimulus he has given to the experimental study of plant variation. Not only has he done a vast amount of this sort of work himself, but he points out very clearly numerous problems awaiting the investigator.

It is remarkable that thus far so little has been done in attempting to produce anew the varieties of cultivated plants by beginning with the wild plant and conducting the work under critical scientific conditions. This is perhaps impossible in the case of our most important plants which have been cultivated since prehistoric times—and of whose original form we are in many cases ignorant, but it surely is a feasible and logical method of procedure in the case of plants domesticated in recent times, as is the case with many ornamentals. There is, I believe, no dissent from the statement that cultivated plants show far greater diversity than their wild progenitors. Is this greater diversity merely due to intensification of differences already possible of discernment in the wild plant, or do really new types appear under the stimuli of cultivation? To use a simple example, *Impatiens sultani,* an African ornamental, was first introduced into cultivation about twenty years ago, only a single color being then known. It now occurs in four distinct colors. Have these arisen under cultivation or were they found as wild sports? A more complex case. *Phlox drummondii* is a native to Texas and not very variable, so far as known only pink, purple and red varieties existing wild. It was introduced into cultivation about seventy-five years ago. There is now a bewildering array of color varieties—both with entire and with fringed petals. In the so-called star of Quedlinburg varieties the central tooth of the fringed varieties is prolonged into a lobe as long or longer than the petal. In the wild form there is apparently no hint of such a character. It ought to be no difficult task to repeat the evolution of these forms under test conditions and thus get a full record of what takes place. Until this is done our picture of the process must remain incomplete. How far extreme conditions as to soil, heat, moisture and other external factors may affect the process of variation, especially permanent variations, is one of great interest and importance. Our wide range of soils and climates gives us unusual opportunity to plan such investigations. To start anew with the wild forms of our most important crops, wheat, oats, corn, beans, potato, etc., is rendered difficult owing to our ignorance of the wild progenitors of these crops. Why these should have disappeared if such is the case is very puzzling. Aaronsohn has recently discovered in the mountains of Palestine what are probably the wild originals of wheat, of barley and of rye. As this country was long ago well explored botanically, the question at once arises—why were not these plants found? Aaronsohn offers a humorously simple explanation, namely, that no botanist ever collects a cultivated plant and no agronomist ever looks at a wild one. Perhaps a similar explanation may account for our ignorance of corn and other American natives in the wild state. A particular interest in knowing the wild form of such plants is to be able to measure the progress that has been made by cultivation. Another is to determine how quickly it may be possible to breed up to the approximate standards of the long-cultivated strains. There is a general belief that great improvements can be made in the early processes of breeding for improve-

ment but that these rapidly and progressively become less and less with each step in advance. This is perhaps true as it is a general law of nature. Yet the improvement made in some supposed cases is vastly greater than could possibly have been anticipated. Thus the gap from Johnson grass to its supposed derivations, such as Kafir, Jerusalem corn, milo, Sumac sorghum and a host of other varieties is so great as to stagger one's belief. Yet the botanical evidence is good enough to warrant critical experimental investigation.

How much further wheat, corn and other long-cultivated plants may still be improved can not be foretold, because we are too ignorant of the potentialities which have brought them to their present development. In any attempt that may be made to redevelop the cultivated forms from the wild forms, two things will have to be considered—first, that various forms of the wild plant may and probably do exist in different regions—and second that even beginning with the same wild form its descendants in different regions will probably vary in different directions. Only on one or both of these hypotheses can we explain the fact that with anciently cultivated plants each region has its own peculiar varieties and types. The problem of the origin of the more marked varieties of the plants cultivated in and since prehistoric times becomes an exceedingly complex one, probably capable of being duplicated only in small part. We must not underestimate the ability of even very low races of agricultural people to improve their cultivated plants. Certainly the Indians developed corn to a very high degree and had some pretty clear ideas regarding its culture. For example, the Virginia Indians made it a point to plant in each hill seed from several different ears.

It seems to me that we too often err on the side of making phenomena appear more simple than they really are. Plants are vastly more complex organisms than our formulated ideas recognize. Many of their phenomena completely baffle us. For example, I might mention what has been called aggressiveness in a plant—namely, its ability not only to occupy and maintain the soil, but to spread and crowd out other plants. This is particularly evident in plants introduced from one country to another. Thus nearly all of our weeds are of old world origin. The same is true of our permanent meadow and pasture plants, where ability to occupy and hold the ground against weeds is essential. In this respect our American grasses and clovers utterly fail before the foreign immigrants. Some other striking instances of the great aggressiveness of an immigrant may be cited. The introduced English violet is said to be the worst of weeds in Mauritius; American cacti are becoming a pest in South Africa; the marvelous vigor and spread of the American waterweed (*Elodea*) under European conditions is well known. Several explanations of these and similar phenomena have been advanced. The commonest one is that the plant is introduced but its fungous and insect enemies are not. Therefore, the plant is released from all handicaps as it were and can exercise to the utmost its inherent energy. A second and related explanation is that every plant becomes held within limits by the competition of other plants in its native land, and very often in the new environment the native plants do not have an equal restraining influence—because they have had to contend with a different set of competitors. A third idea is that any organism with the ability to

spread at all becomes more energetic through the constant mixing of blood of the advancing population. All these ideas are interesting, but difficult, if not impossible of experimental proof. The last suggestion receives some support from the fact that many weeds and other organisms "peter" out after they have ceased to spread. The recent examples of the Russian thistle and the prickly lettuce are familiar cases. Such phenomena may be due wholly or in part to increase in enemies—but in many cases like the two cited there is no iota of positive evidence. I think we ought to give such phenomena more consideration, as they reveal traits in plants that transcend all of our stereotyped and inadequate theories. The old gardener often treats his plants as if he regarded them as sentient beings. Perhaps we err in considering them too much as machines.

I have touched thus much on the botany of our cultivated plants and their origin and behavior under domestication because I believe that there lies here a great field for botanical and agricultural advancement. It matters not what we call this phase of botany—its successful prosecution demands both broad and intensive botanical training. It requires at least a good knowledge of systematic botany, of plant physiology and of the theories and principles of plant breeding and plant evolution. One must at least know all the botany possible of the plants he is immediately concerned in breeding, lest he be lured into needless error. Among his many experiments, Mr. Oliver has made some very interesting hybrids of *Poa arachnifera,* the Texas bluegrass and Kentucky bluegrass, a circumboreal plant. His culture soil was presumably sterilized, yet mixed with his hybrids were plants of Canada bluegrass, *Poa compressa.* One enthusiastic Mendelist was jubilant over the supposed discovery of the origin of this grass and at once proposed an additional series of experiments. Now *Poa compressa* is a European species—and the securing it by crossing a Texas species with common bluegrass was certainly a startling phenomenon. Fortunately or perhaps unfortunately, some of the other supposed hybrids in the lot turned out to be other grasses, including timothy and sweet vernal grass, so that the source of the error was evident. It points, however, clearly to the necessity of the scientific breeder knowing the systematic botany at least of the group he is working with.

I well recall that when I first began to study plants I promptly found about a dozen species of red clover—at least they were different from each other. It took a long time to teach me that in plants there were differences and differences, some of which should be taken seriously and others ignored. In general, I was taught that any differences that existed in closely related cultivated plants were to be ignored, but in wild plants they would usually have to be considered. It is really very fortunate for the cultivated plants that systematic botanists have not taken their differences seriously, otherwise we would have chaos indeed. It is unfortunate that the conservatism which most systematic botanists exhibit toward cultivated plants should not be exhibited as well toward wild plants. If more attention had been given to the cultivated plants, think what a vast host of reputed wild species would have escaped the pangs of christening. There used to be hope that after a while all the species would be described—so that systematic botanists could devote themselves to deeper studies. But alas, it seems only necessary to make finer distinctions to reveal a wondrous display of so-called species where

none was seen before. It, therefore, seems inevitable that a new race of systematic botanists will have to be developed to devote themselves to cultivated plants—for it needs no seer to predict that many generations of botanists will be needed to define and describe all the minute forms in nature which it is now proposed to call species. The fatuity of such work, however, will defeat itself. As a matter of fact, the naming of a species is an interpretation of facts just as our theories of variation are interpretations of the same or very similar facts. For both purposes we need far more of the facts that can only be gathered in rigid pedigreed breeding experiments. Botanists have too long neglected the most vital features of botany to the theoretical evolutionist and to the commercial breeders. We have developed to a high degree nearly every phase of the subject that does not touch industry—and have neglected those of most practical import. Our hope of aiding the art of agriculture is in developing its underlying sciences. Too many of us have reversed this idea and think to help the sciences of agriculture by devoting more attention to its art. But gardeners do things with plants that are the despair of the physiologist, and there always will be vastly better farmers than the scientists.

The matter of botanical instruction in all schools is to a large extent a matter of fashion—and the fashion is usually set by the larger universities, where no attempt is made to give botany an industrial trend. There has thus been developed a splendid lot of texts on morphology, embryology, systematic botany, physiology, etc., but none of this material has been presented in its agricultural bearing, and consequently the field of botany in agriculture has not been clear. At the present time it has neither direction nor aggressiveness. What we really need to work on is the science of the breeder's art and the science of the gardener's art. At present, the art is far in advance of the science. In fields where the agricultural art was not highly developed—notably pathology and bacteriology—the botanist has accomplished great things. Greater things remain in the botanical fields he has thus far so largely neglected. If we pursue agriculture or any phase of it without devoting our science to it, we can at most become expert farmers. By devoting our science to agriculture and having faith in its potency, no man can foretell the outcome.

I have endeavored to indicate what I regard as the most promising lines for botanical work to advance agricultural progress. The routes that the investigators have followed and are following along these lines furnish the natural and best possible chart upon which to map botanical courses in agricultural schools. These courses should be fashioned as far as possible to promote interest in the botanical problems of agriculture, rather than those with little or no agricultural contact. To me it seems as if the great field that is at present open to us is that of determining as fully as possible the potentialities of our principal crop plants so that they may be utilized to the utmost.

In some ways we might compare our present knowledge of plant species or their subdivisions to the knowledge of organic chemistry fifty years ago. At that time it was believed that organic compounds could be formed only by vital processes. In a similar way there exists among biologists the more or less unformulated idea that species and subspecies are the result of forces beyond our command; that we can study their evolution but can not control the processes. It seems to me that the results obtained by the cultivator of plants

and the domesticator of animals virtually contradict this idea, enough so at least that there is good basis for De Vries's bold prediction:

A knowledge of the laws of variation must sooner or later lead to the possibility of inducing mutations at will, and so of originating perfectly new characters in plants and animals. And just as the process of selection has enabled us to produce new races, greater in value and in beauty, so a control of the mutative process will place in our hands the power of originating permanently improved species of animals and plants.

C. V. Piper

Washington, D. C.,
March 5, 1910

SCIENTIFIC NOTES AND NEWS

Following the advice of its advisory board, The Wistar Institute of Anatomy is about to extend its work by the establishment of a department of embryology. At a meeting of the board of managers of the institute, held May 27, a professorship of embryology was established, and Professor G. Carl Huber, of the University of Michigan, was called to this chair. Professor Huber will begin his work at the Wistar Institute in 1911.

Dr. William Colby Rucker, of the United States Public Health and Marine-Hospital Service, has been granted leave of absence for one year to accept the position of health commissioner of Milwaukee.

Dr. C. F. Lorenz, formerly of the Queen's University, Kingston, Ontario, has entered upon the duties of his position as associate physicist in the Physical Laboratory of the National Electric Lamp Association. Mr. A. G. Worthing, of the University of Michigan, and Mr. M. Luckiesh, of the University of Iowa, have also accepted appointments in the laboratory.

Mr. Jerome D. Greene, secretary of the Harvard College Corporation, has been appointed superintendent of the Rockefeller Institute for Medical Research and its new hospital.

Dr. Aleš Hrdlička has been promoted to a curatorship of anthropology in the U. S. National Museum. He has started for South America to carry on some work in Peru and Bolivia and to attend the Congress of Americanists.

Columbia University has conferred its doctorate of science on Sir William Henry White, for many years director of naval construction of the British navy, and on Dr. W. J. Mayo, the eminent surgeon of Rochester, Minn.

On the occasion of the installation of the Duke of Devonshire as chancellor of the University of Leeds, the degree of doctor of science was conferred on Lord Rayleigh, Sir Clements Markham and Professor William Osler.

Lord Rayleigh has been promoted from a corresponding to a foreign member of the Berlin Academy of Sciences.

Dr. W. Solomon, professor of geology at Heidelberg, has been elected a foreign member of the Academy of Sciences in Milan.

The two eminent pharmacognosists, Professor Arthur Meyer, of Marburg, and Professor A. Tschirch, of Bern, were elected honorary members of the American Pharmaceutical Association at the recent meeting in Richmond, May 3–7, 1910.

Dr. Rollin D. Salisbury, professor of geographic geology at the University of Chicago, has been elected president, and Dr. Henry C. Cowles, assistant professor of plant ecology, first vice-president, of the Geographic Society of Chicago.

The American Philosophical Society has appointed its president, Dr. William W. Keen, to represent it at the Centennial Jubilee of the University of Berlin to be held in October next.

Professors Sollas and Bowman have been appointed university representatives from Oxford University to the eleventh International Geological Congress, to be held at Stockholm.

The Barnard Medal was awarded at the commencement exercises of Columbia University to Professor Ernest Rutherford, director of the physical laboratories, University of Manchester. This medal, established